Discussion before the Wireless Section, 2nd March, 1932.

Mr. P. K. Turner: I shall deal first of all with Part 2 of the paper. The author's proposal (page 120) of awarding an overall figure of performance to broadcast receivers seems to me to be an impossible one. For example, although his overall performance figures show the 3-valve to be better than the 4-valve set, we have to face the fact that the question which of those sets is better for a particular user depends on what that user wants. In other words, it is impossible in principle to give any such overall figure. The class " D " receiver referred to on page 120 is designed for distant reception, i.e. field strengths below 2·5 millivolts per metre, and I suggest that the signal strength of 1 millivolt per metre which is applied is much too high. A receiver of that type should give its full output at 25 microvolts per metre. I do not agree with the statement (page 121): " In view of the crowded nature of the frequency allocations of broadcasting stations, it is considered that the upper audio-frequency limit which should be catered for is 5 kilocycles per second." A good deal of propaganda work is being carried out at present with a view to convincing the ordinary listener that he does not require a higher frequency limit than 5 000 cycles per sec., the maximum which can conveniently and cheaply be provided for sound films and gramophone records. In my opinion, however, a good frequency response characteristic up to 8 000 or 9 000 cycles per sec. is necessary and can be obtained, in spite of the state of the ether. While I support the author's idea of judging selectivity by the amount that one can cut out over a wide frequency band, as illustrated in Fig. 7, I think that the scales of this diagram should be modified in the way described below. I would also press for a wider frequency band in Fig. 8, as it is not fair to base the figure for the audio-frequency response of a first-class receiver on a range of 50 to 5 000 cycles per sec. Further, any local irregularity in the response curve of a loud-speaker in the neighbourhood of 400 cycles per sec. would have an altogether disproportionate effect on the result as obtained from the author's calculation. If a loud-speaker were tested in the way suggested, a 100 per cent resonance at 400 cycles per sec. would result in the reactangle ABCD being doubled and its efficiency figure halved. I therefore suggest that the output used for purposes of comparison should not be that at 400 cycles per sec. but should be that corresponding to the mean height of the curve: this would be independent of local irregularities. My second point in connection with Fig. 8 is that I think the units of the vertical scale should be decibels. For example, the audio-frequency response of the 4-valve receiver of Fig. 14 is reduced to one-tenth of the full value at 5 000 cycles per sec. If, however, between 2 000 and 5 000 cycles per sec. the response had dropped to one-hundredth of the full value, although the additional area would not have been greatly different from that shown the actual change in performance would have been very large. The use of a decibel scale would really make the area of the rectangle ABCD disappear from the problem altogether. It is a pity that all three of the receivers the results of which are shown should be so bad. With regard to Part 1 of the paper, I should be glad of further details of the valves used in the radio-frequency amplifier, and of the adjustments which are necessary in order to obtain a linear relationship. Instead of the small variable condenser to give small changes in frequency for selectivity determinations, it would perhaps be preferable to have part of the inductance in the form of a small variometer, because this would enable one to make a definite percentage change no matter what the main frequency setting happened to be. A modulation meter based on approximately linear rectification is much easier to handle than one based on approximately square-law rectification. I should be glad of further details of both the current divider and the potential divider, as I have found such instruments very difficult to make with any pretence to accuracy. They have to be extremely well screened, and usually screening upsets the calibration on account of

capacitance effects. Table 1 shows the corrections to be applied to the modulation percentage owing to the fact that the input was measured with an R.M.S. meter (thermo-junction), but I am surprised that this difficulty was not evaded by making the essential measurement with a valve-voltmeter, which reads mean volts and needs no correction if its calibration curve is linear. My own experience in less accurate work is that the convenience of the radio-frequency voltmeter outweighs in most cases the theoretical accuracy of the thermo-junction. With regard to the measurement of harmonics, it seems to me that a weak point in the author's method is that what is actually obtained is the sum of the R.M.S. values of all harmonics. Thus a receiver with an output of 10 per cent of second harmonic and nothing else would give exactly the same rating as another receiver with outputs of 5 per cent each of the second and third harmonics: yet it is curious that the one with the 10 per cent of second harmonic seems much more offensive to listen to. As far as I can judge, the effect of harmonics on the ear is more a function of the amplitude or percentage of the largest harmonic present —if it is of a low order (second or third)—than of the sum of all the harmonics. We therefore need an instrument which will measure the amplitude of the maximum harmonic rather than the sum of all the harmonics present.

Mr. F. S. Barton: I should like to know the size of cabin A, as it occurs to me that manufacturers and others may sometimes wish to submit other than broadcast receivers for testing. The author's dummy aerials—of which more details would be appreciated, especially for those not within the ordinary broadcast range—seem to me to be arranged specifically for the type of receiver that has aerial and earth connections. How would he test portable broadcast receivers with frame aerials, and the commercial type designed for a balanced array in which both sides are at high-frequency potential with respect to earth? Has the author had any experience with standard-signal generators, in which the whole of the generating, measuring, and attenuating equipment is in a screened box, and the receiver remains unscreened? If so, what are the faults of that type of arrangement? Passing now to Part 2, with regard to the author's definition of selectivity there are two features of a selectivity curve in which we are interested. One is the width of the flat top, and the other is the width of the resonance curve near the skirts. The width of the flat top, which governs quality of reproduction, could be given as the total width 6 decibels down from the maximum. To specify the bottom of the curve, I suggest that the lower width should be given at something like 50 decibels down from the maximum. If the curve were only some 17 to 19 kilocycles wide there, one would expect fair freedom from interference. A recent article* by W. B. Snow, of the Bell Telephone Laboratories, describes a series of tests on the range of frequencies necessary for satisfactory musical reproduction, which embodies the opinion of some 20 or 30 trained observers. Briefly, the results are that whereas audible frequencies as low as 40 cycles per sec. are produced by musical instruments, reproduction down to 60 cycles per sec. is considered to be almost perfect. The transmission of the highest audible frequencies is necessary for the perfect reproduction of sounds produced by musical instruments, mainly because of the noises accompanying the musical tones. An upper cut-off at 10 000 cycles per sec. had a slight effect on the quality of most instruments, but a cut-off at 5 000 cycles per sec. had an appreciable effect on all but the large drums. The quality of reproduction of orchestral music continued to improve materially when the lower cut-off was extended to 80 and the upper to 8 000 cycles per sec. Reproduction of the full range, which in these experiments extended from 30 to 13 000 cycles per sec., was preferred to any limitation of band width. For the reproduction of noises the highest audible frequencies were required; even a cut-off at 10 000 cycles per sec. caused appreciable reduction in the naturalness of common noises. The results seemed to necessitate no radical revision of the qualitative ideas which have been accepted by accoustical engineers for some years; their value lay rather in the quantitative corroboration which they supplied. I wish, then, to register a strong plea for a frequency range of at least 80 to 8 000 cycles per sec.

(*Communicated*): No provision seems to have been made by the author for testing receivers having an automatic volume control, nor does he specify any performance factor to cover this feature. I think automatic controls will become increasingly common on broadcast receivers in the near future. In regard to the tests and specification proposed for selectivity, these do not seem to me to cover the case of a receiver depending partly for its selectivity on the demodulation effect at the detector. This case could perhaps be dealt with by tuning the receiver to a fixed carrier and determining its selectivity by means of a second variable carrier modulated at 50 cycles per sec. as in the author's method. The audio-frequency power output resulting from this variable carrier could then be measured, and the selectivity curve plotted as is suggested in the paper.

Mr. M. G. Scroggie: Is there any particular object in selecting such a low value as 60 ohms for the output resistor shown in Fig. 2? This appears to be a rather inefficient arrangement, without a step-down transformer or other device for adapting it to the valve, and in my opinion it would be likely to encourage overloading. I should like to ask the author whether his arrangement is capable of covering, say, the medium wave-band at one sweep of the condenser. The total wave-band covered by the apparatus is very large and requires a number of coils, but whatever disadvantage this may constitute in manipulation is largely removed by ability to cover the most usual bands without coil-changing. The general practice now is to do away with screened cabins, and I should like to know whether they are really necessary in the author's equipment. In commenting on his previous paper* I remarked that in American practice only one cabin was used. American radio engineers now dispense with cabins entirely, even for signals of 1 or 2 microvolts. With regard to the method of measuring distortion, has the author tried the method of using a filter to cut out the fundamental, leaving only the harmonics? One measures first the harmonics

* *Journal of the Acoustical Society of America*, 1931, vol 3, p 155

* *Journal I E E*, 1930, vol 68, p. 475.

alone, and then the fundamental plus harmonics, but with an attenuator in circuit to bring the total output to the same level in each case. The attenuator can then be calibrated in percentage harmonics. In my opinion the service areas defined (" A," " B," etc.), however suitable when first proposed, are not now representative delimitations. Only a small part of the whole gamut of field strengths existing in practice is split up into these classes, and the proposed additional area " D," which includes the aim of most modern receivers, does not quite restore the balance. A logarithmic classification is surely more in accord with modern practice. I notice that the author tests receivers without their associated loud-speakers, and in this connection I would point out that 90 per cent of present-day designs incorporate the loud-speaker in the receiver, and in many cases the characteristics of receiver and loud-speaker are complementary. I should like to know what precautions it is considered possible to adopt in order to avoid treating such receivers unfairly. I am glad to note that the author recognizes the conditions that actually exist, and fixes his upper frequency limit at 5 kilocycles per sec. Idealistic aspirations after 10 kilocycles per sec. acceptance should be reserved until international commissions have succeeded in reducing the present overcrowding of frequency channels. Turning to the method of defining selectivity, though the author rightly rejects the method which considers the ratio of response at resonance to that 5 kilocycles per sec. off tune, his objections do not apply if the frequency difference is increased to 9 kilocycles per sec.—the spacing between adjacent channels—and this ratio is much less tedious to derive than the one he advocates. In view of present tendencies in receiver design, demodulation effects should certainly be taken into consideration in assigning a figure of merit for selectivity. The author's preference for a linear power-output scale is surely contrary to all accepted practice in communication engineering. The advantages of a decibel scale are too well known to need emphasis. The definition of audio-frequency response (Fig. 8) is unsatisfactory, for this reason: a fairly level characteristic, marred by one bad resonance, would actually be much the same whether the resonance were at 400 or 450 cycles per sec.; yet widely different figures of merit would be assigned according to the plan suggested, owing to the arbitrary fixing of the datum level at 400 cycles per sec. I agree with Mr. Turner that it is almost impossible to place a useful interpretation on overall figures of merit for broadcast receivers. For my own part, I prefer the " 24·9 "-receiver (Table 2) to the " 41·2 "-receiver. Although the selectivity of the former is awarded the encouraging figure of 95·5 per cent, its selectivity is not nearly adequate for present-day conditions; while the audio-frequency response, for which the low figure of 45 per cent is awarded, is tolerably good according to commercial standards. The difficulties of rating broadcast receivers seem almost insuperable, and the author is to be congratulated on tackling the problem so boldly.

Mr. W. D. Oliphant: The subject of the paper is of vital importance to the radio industry, and I look forward to the day when a standardization committee will be formed to review the whole matter and lay down specific definitions and methods of test. American engineers have already realized the existence of this need, and their deliberations are to be found in the 1931 report of the Standardization Committee of the Institute of Radio Engineers. The compilation of a set of standards will present many difficulties, but an equally difficult task will be the setting of limits within which the tested apparatus must fall. In view of the complexity and expense of the possible tests, it will be impossible for individual manufacturers to install the necessary apparatus, and I have in mind the establishment of a central proving station whose duty it will be to carry out the necessary tests on the submitted apparatus; then, when the tests yield data satisfactory to the manufacturer concerned, they can be published and thereafter production can be checked against the standard-tested article by simple comparison tests. This proving station, which would be supported by the

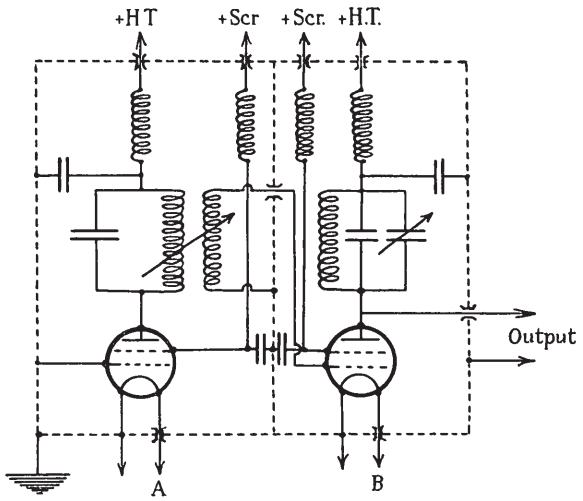

FIG. A.

industry, would also carry out research on subjects of interest to the industry as a whole. Passing on now to the paper, the author's primary object was to localize between the input terminals of a receiver a known minute modulated radio-frequency voltage. We must consider not only circuit details but also the mechanical arrangement of the entire system, as on this factor depends the elimination of a number of stray voltages whose presence would defeat the object in view. A comprehensive analysis of this question has been given by J. R. Bird.* I should like to ask the author whether he has used the dynatron oscillator as either a modulated radio-frequency or a beat-frequency oscillator. The results which I obtained some time ago on the simple dynatron oscillator confirm those recently published by F. M. Colebrook.† I further found that this type of oscillator could very easily be modulated by applying the modulation voltage to the control grid. These experiments led me to suggest the design for a beat-frequency oscillator shown in Fig. A. It consists of two dynatron oscillators A and B; A oscillates at a

* " The Design of Radio-Frequency Signal Generators," *Proceedings of the Institute of Radio Engineers*, 1931, vol 19, p. 438.
† *Experimental Wireless*, 1931, vol. 8, p. 581.

fixed frequency, while the natural frequency of B can be varied within 10 kilocycles per sec. of that of A. Further, B is modulated at the frequency of A, so that the output from B is of the desired beat frequency: this is rectified, filtered, and amplified in the usual way. Has the author investigated any experimental methods of checking attenuator output voltages? The best method appears to be to connect two dissimilar attenuators in cascade; on increasing the attenuation of one and decreasing that of the other by the same amount, any change of output could easily be detected on a calibrated receiver. The validity of the method rests upon the fact that the two attenuators would not have compensating errors. I am very sceptical about calculated outputs, as we do not know what stray effects are present. In the case of mains receivers we are faced with another possible input, namely via the mains leads and high capacitance between the mains transformer and the rest of the wiring. Some test will have to be devised to investigate this effect, and I should be glad of the author's views on the matter. In conclusion, I would point out that the modulation percentage used by the Americans is 30, and not 40 as stated in the paper.

Dr. R. L. Smith-Rose: The first part of the paper describes the improvements and modifications in the apparatus which is in use at the National Physical Laboratory for calibrating receivers, and outlines the alterations which have been made in order to cope with modern demands. While from the description the screening may seem to be rather elaborate, it does not add in any way to the difficulties of carrying out the measurements. We have a screened cabin (a 6-ft. cube) into which an observer can get with all his apparatus, and the receiver under test is taken into a screened room of considerable dimensions; there is no difficulty in operating the whole installation. A number of problems which probably do not occur to the ordinary technical man at the works when catering for the testing of his own receivers, are introduced by the enormous range in both wavelength and sensitivity which has to be dealt with by the apparatus. Naturally its use for broadcasting wavelengths is very important, but it has also been used for testing short-wave receivers down to 7 metres, at which wavelength the apparatus behaved quite well. Turning to Part 2 of the paper, the circumstances which have led up to the development of the proposed method of specifying performance are as follows. Manufacturers and others have been in the habit of sending receivers to the National Physical Laboratory with a letter asking that these should be "tested," but without stating the nature of the test required. In replying, to put the matter on a concrete basis, we had roughly to outline the tests we proposed to make, viz. sensitivity test, selectivity test, and audio-frequency test. We never carried out one of these tests apart from the others, since the results of such a test might easily have been used to misrepresent the real performance of the receiver. We therefore had to cater for the whole series of tests, and the scheme set out in the paper was devised by the author with a view to simplifying the procedure. We hope that the proposals may perhaps serve to inaugurate a committee of this Institution or of the British Standards Institution which may in due course produce a practical specification for the testing of receivers. With regard to Mr. Turner's query as to why the author selected the 400-cycle ordinate in Fig. 8 for the definition of audio-frequency response, 400 cycles per sec. was tentatively adopted as the standard audio-frequency. In most of the audio-frequency characteristics we have come across there has been no substantial peak or dip at that frequency, so that the possible error envisaged by Mr. Turner does not seem likely to occur with the types of receivers at present available. The great advantage of standardizing the ordinate, of course, is that it does away with the necessity for calculating the mean value of every curve measured. One of the criticisms which might reasonably be levelled against the use of a band of more than 5 000 cycles per sec. is that modern receivers do not appear to take much account of frequencies above 5 000. As regards the criticism of the choice of the response curves given in the paper, I would state that Fig. 16 is the actual curve obtained from a modern commercial receiver. Taking the published response curves of three commercial receivers at present on the market, and working on the basis of the paper, I find that the audio-frequency responses of these three lie between 46 and 63 per cent. I should like to refer in conclusion to the question of selectivity characteristics. As has been pointed out, our method of recording selectivity characteristics is just the inverse of that adopted by the Institute of Radio Engineers. I do not like the inverted resonance characteristic, in the first place because it is not the type we have been used to, and secondly because it does not seem to represent the conditions as we want to know them. In talking of the selectivity of the receiver we do not necessarily want to know how strong an interfering signal must be to produce the same output as was obtained from the wanted station. The problem is rather that of determining how much output an interfering signal produces in relation to that of the wanted signal when the two incoming sets of waves are both of the same field strength.

Mr. W. J. Brown: Has the author made any direct measurements of low radio-frequency voltages as a check on the potentiometer calculations which he employs in his signal-measuring device? I have in mind that in the design of radio-frequency attenuators or potentiometers very serious errors may be introduced on account of stray capacitance currents flowing from the oscillator through the attenuator to the receiver. These stray capacitance currents produce quite an appreciable voltage-drop in the inductance and the earth-return leads, and the error may amount in some cases to 10, 50, or 100 per cent. I feel that any figure of merit for a broadcast receiver should include the acoustic performance of the loud-speaker and cabinet. It is useless to aim at an ideal audio-frequency response curve for the receiver alone, when this is liable to be entirely spoilt by the usual irregularities in response of the average commercial loud-speaker. In particular, when testing receivers employing pentode output valves it is very misleading to use a resistive load, as this takes no account of the variation in amplification of the pentode when working into a loud-speaker whose impedance varies widely with the frequency. Turning to the various

factors which contribute to the figure of merit, it seems to me that the sensitivity of the receiver is dismissed rather summarily by merely placing it in one of a number of classes. The sensitivity of a receiver often varies considerably over the wave band, particularly in the case of gang-tuned circuits. In such cases a great deal of development is often required to establish correct ganging, so as to obtain uniform sensitivity over the wave band. I think the performance of the receiver in this respect should be acknowledged in the figure of merit. As an example, a badly ganged superheterodyne receiver may have a sensitivity of 20 microvolts at 300 metres, and of 2 000 or 3 000 microvolts at 220 metres or 500 metres. On the other hand, a similar set of good design would have a sensitivity varying between 20 and 50 microvolts over the entire range. The author's suggestion that receivers should be tested at their maximum rated output seems to be subject to the drawback that when the output is approaching saturation the audio-frequency response curve will appear flatter—and therefore actually better—than it really is. I suggest a figure of one-half the rated output as being a rather more satisfactory compromise. With regard to the selectivity, the author suggests that this should be measured at constant input instead of constant output, but I do not feel that this gives the kind of comparison we need. The purpose of a selective receiver is to receive signals of the order of 0·1 mV per metre from a distant station the frequency of which is spaced only 10 or 20 kilocycles per sec. away from that of a local station giving a signal strength of 100 mV per metre. The input thus varies in the ratio 1 000 : 1. The output, on the other hand, is naturally adjusted by the user to a more or less constant value, sufficient to give a comfortable volume of sound in his room. Thus we can only simulate the actual conditions of listening if we test with constant output and vary the input accordingly. A further point is that if we have a curve of output at constant input we get the sort of thing the author depicts in Fig. 13. If we wish to find how many kilocycles per sec. we have to detune in order to decrease the response 1 000 times, so as to get the conditions of receiving a distant station from a point close to the local transmitter, the scale of Fig. 13 becomes so small as to be unreadable; in fact, the lowest output we can estimate from Fig. 13 is 10 milliwatts, which corresponds to only $\frac{1}{50}$ the peak output or $\frac{1}{7}$ the peak output voltage. A sevenfold reduction in voltage is very small compared with the 1 000 : 1 ratio which actually exists between the signal strengths of the local and the distant station, and it is impossible to say from Fig. 13 how far it would be necessary to detune the receiver from the local station before a distant station could be received (it looks as though it would be of the order of 100 kilocycles per sec.). I should like to refer to the high figure of 95·5 per cent which the author gives for the selectivity of the arrangement shown in Fig. 13. This is obviously not a very selective receiver, as nearly 100 kilocycles per sec. detuning is necessary for a 1 000 : 1 ratio, and in a modern receiver of the superheterodyne type it is only necessary to detune 10 kilocycles per sec. to allow for the same ratio. In other words, modern receivers can be produced having 10 times the selectivity of the arrangement shown in Fig. 13, and the figure of merit of 95·5 which has been allotted to this receiver does not give sufficient scope for improvement.

Mr. L. B. Turner: Many of the previous speakers have referred to the undesirability of an omnibus figure of performance for a broadcast receiver, and I thoroughly agree with their objections to such a figure. Also, one of the three terms upon which the figure of performance is based is selectivity, and I should like to criticize the manner of defining this which is explained in Fig. 7. The method adopted by the author leads to the selectivity being expressed as x/y, where x and y are respectively the areas CDEF and CKLD. These areas approach equality as the selectivity approaches perfection. I think it would be preferable to measure the small difference of these two nearly equal large quantities, and perhaps define the selectivity as $y/(y - x)$. The unrejected residuum has to be dealt with, and there is a practical 2 : 1 relation between two receivers whose selectivities the author would express as 99 and 98 per cent.

Mr. A. H. Cooper (*communicated*): To what does the author attribute the presence of 10 per cent of harmonics in the output of his 3- and 4-valve sets even near zero output? Is it due to impurity in the low-frequency oscillator, or in the modulator, or does it originate in the detector of the receiver? I cannot understand why his method of measuring selectivity is more representative than that adopted by the Institute of Radio Engineers. The author's method gives the product of the ordinate of the resonance curve and the detector efficiency at the correspondingly decreased input. Thus, in two similar receivers, one with a square-law and the other with a linear detector, his method will give a better selectivity figure for the square-law detector, whereas it is common knowledge that, owing to demodulation, the linear detector is the better. This is quite in addition to the difficulty already mentioned by another speaker of measuring the output to a sufficient attenuation to be of any use. It is idle to suggest that this curve represents actual fact: in practice the set is used to receive a wanted signal in the presence of a neighbouring unwanted one. The wanted signal will load the detector so that its efficiency is that of the centre of the scale, and its changes at other inputs do not concern us directly. I can see no way of stating the selectivity of two widely different receivers except in terms of two inputs, representing the wanted and unwanted signals. A trivial point in the proposed standardization scheme is the division between the medium-output class (3) and the large-output class (4) at 1 watt. The output of many typical 3-valve mains sets ranges from 0·8 watt to 1·2 watts: some of these sets will fall in one class and some in the other, whereas in point of fact they are all very similar. I suggest that the point of division should be raised to 1·5 watts. Finally, I should like to utter a word of warning against the apparent safety of elaborate screening. A radio-frequency generator of large dimensions (and the author's is of a very large size) is difficult to check; its output is often only partly controlled by the attenuator. The voltage-drop in the various earth paths is considerable and cannot be checked completely by varying the

attenuator ratio and current in opposition. The laboratory where I work has in the past 2 years abandoned the use of large screens in favour of rational screening of small size, in which earth paths can be isolated and allowed for. By this means a generator can be made to work in a week instead of 6 months. A large generator in a screened room often gives less accurate results than a small portable generator, which is so easy to use and cheap to make or buy that it will be used throughout the design of a receiver, and the product will be better in consequence.

Mr. R. H. Nisbet (*communicated*): If, for a measure of audio-frequency response, a quantity is required which will be unity for perfect response, why does the author not choose the ratio of the unshaded area *under* the response curve of Fig. 8 to the area ABCD? Is the reason that the figure so obtained would be too near to unity for all receivers, so that the worst receivers would appear to differ by too little from the best? If this be the case the construction of the author's formula, with its arbitrary constant K, may well be justified. Why not extend the principle to the definition of selectivity, where the author's plan gives high figures of merit even for very unselective receivers? I should like to propose that the level of the line BC of Fig. 8 should be that of the highest peak. This would have the undoubted merit of penalizing receivers with the narrow and very peaked type of resonance curve, which is often associated with the combination of receiver and loud-speaker. In view of the possibility of demodulation of one received signal by another, a subject on which the last word has not yet been said, is it legitimate to define selectivity in terms of unmodulated waves? The definition of selectivity must be based upon the nature of the audio-frequency output when the receiver is tuned to a modulated transmission while subjected to interference from other carriers—modulated and unmodulated. At what level of retroaction would the author test a receiver for selectivity and audio-frequency response? There is still scope for considerable variation inside one of the type classifications, especially in type "D." Who would classify the receiver—the manufacturer or the tester? How would a receiver be tested which was claimed to be suitable for all four service areas? As regards the numbering of the classes, I think a system should be avoided in which a 3-valve mains set might be classified as "C3," while a simple crystal set might be described as "A1."

Dr. E. H. Rayner (*communicated*): The paper deals with a subject of considerable public as well as technical interest. There are many cases in which receiving sets of the more powerful and expensive types are installed in churches, schools, and village institutes. The people responsible for choosing such apparatus have very little to guide them, except perhaps the advice of friends or a certain amount of experience of one or two receivers which will generally be obsolescent owing to rapid progress in methods of operation, new design of circuits, valves, and loud-speakers. From a manufacturer's point of view the amount of advertising is in such circumstances probably as important as quality as an aid to selling apparatus. It would obviously be much easier to spend money wisely if a graded standard of performance were instituted. The question as to what type of receiver should be installed in any given instance depends upon the distance of the particular locality from the wanted station. In this country there are some 10 000 schools which are potential users of good-quality receiving sets. At present probably less than one-quarter of these are provided with receiving equipment, and much of the apparatus now in use is not up to the present-day standard. A recognized scheme of type-testing of receiving sets would not only be helpful to those who are entrusted with expenditure of money of a public character, but it would also be a valuable educational influence for both manufacturers and users of equipment. Several speakers have mentioned the desirability of including tests up to a frequency of 8 000, and it seems that even the most experienced radio engineers have little definite knowledge of the frequency characteristics of commercial receiving sets. I agree with the author that there is little object in testing at 8 000 cycles per sec. if practically nothing of such a frequency is included in the output. The results of a type test, even if—as is desirable to begin with—the standard of performance required is a low one, cannot fail to be of valuable assistance to both the user and the manufacturer. Type tests would help to create a demand for good-class equipment. One such test would probably cost less than a page of advertisement space, of which several are taken in the weekly and monthly Press by many firms.

Mr. P. G. A. H. Voigt (*communicated*): The paper suggests standards which may be difficult to change once they have been generally adopted, and it is therefore desirable to examine any possible alternatives to them. I think that a wider frequency scale should be adopted. "Grasshopper" noises are greatly reduced by eliminating frequencies in the immediate neighbourhood of 9 000, so that a system cutting off sharply at 8 000 will exhibit but little of this class of interference, while retaining a clarity which cannot be obtained with a system cutting off at 5 000 cycles per sec. With regard to classification, I suggest that the number should be placed first and the letter last, for according to the author's system a "C3" set is better than one classed as "A1." I am glad to see that he includes the "purity factor," which becomes increasingly important as the frequency scale is improved. I suggest that this should also be measured at a lower frequency than 400, say at 100, as a measurement at 400 cycles per sec. may not disclose whether the output choke or transformer is capable of handling a strong, low note without introducing serious hysteresis or saturation distortion. In my opinion the percentage fault should be stated rather than the percentage perfection. An unscrupulous salesman could easily persuade the gullible public that the difference between his set, which is 95 per cent selective, and a rival set with a selectivity of 99 per cent, is too small to make any difference. If these two sets were described respectively as 5 per cent and 1 per cent non-selective, this would not be so easy. I consider that the selectivity and response curves should not be drawn on a power scale, but on either a voltage or a decibel scale, using suitably modified values for K. I agree with Mr. Turner that it is undesirable to make use of a fixed frequency in order to obtain the height of the rectangle ABCD (Fig. 8) for the low-frequency

response figure; the mean of two octaves, say from 250 to 1 000 cycles per sec. or from 500 to 2 000 cycles per sec., would be easier to estimate (and more appropriate) than the mean over the whole scale. The question of including the loud-speaker in the calibrations has been raised, and I think that this should be done when the set is intended to be used with a particular loud-speaker, but the figures should be stated separately. The total forward sound radiation can be obtained with sufficient accuracy by taking three response curves, one on the centre line, one 30°, and one 60° off the centre line, and " weighting " the results according to the area which each belt represents. With suitable gear, a response curve covering the whole scale can be drawn automatically in less than 3 minutes; thus the three curves can be drawn and the total result estimated in less than ½ hour. Whether the purity factor of the loud-speaker should also be measured is a matter for debate. The author's apparatus for purity-factor measurement, together with the microphone and amplifier used for the response measurement, should be satisfactory for this purpose. If this latter test is to be of any value it should be made at several different frequencies.

Mr. H. A. Thomas (*in reply*): Mr. P. K. Turner points out that the specification may give a better overall figure for a receiver having a poor sensitivity than for a receiver having a much higher sensitivity, and he says that he personally would prefer the more sensitive receiver. He also states that the question of which is the better of two receivers is determined by the needs of the particular user. That is precisely what the suggested classification does, in so far as it permits of comparative analysis being made between two different receivers. Doubts may arise as to the feasibility of an overall figure of merit, but I cannot see where the suggested specification fails to bring to light the relative properties of receivers. The field-strength classification is essential if tests are to be made with any rapidity, and it appears to me to be the only method of obtaining comparative results. Results of selectivity, audio-frequency response and purity factor, given for all sensitivity settings of a receiver, would be so cumbrous as to be worthless for quick-comparison purposes. The field-strength figures are adopted as a result of measurements on broadcast transmission-service areas, and they cater for the majority of receivers designed to operate within these areas. Mr. Turner regards the highly selective and sensitive receiver as the normal type, whereas I consider the receiver designed for use within a service area to be more representative of the average present demand.

Several speakers refer to the upper audio-frequency limit of 5 kilocycles per sec. as being too low. I agree with Mr. Barton that a cut-off at 5 kilocycles per sec. does definitely detract from the reproduction of much orchestral music and speech, but I maintain that this upper cut-off figure is governed by the international frequency allocation and that, so long as this is as low as 10 kilocycles, there is no merit in aiming at anything higher. Furthermore, the remarks of Dr. Smith-Rose show that few commercial receivers give a response at frequencies above 5 kilocycles per sec.; hence the limitation is more academic than practical with present designs.

Mr. P. K. Turner suggests that the line BC in Fig. 8 should represent the mean of the area and not the value at 400 cycles per sec. In the original specification this was done, but it involved the calculation of this mean value for each curve. Since our experience was that no receiver tested showed any marked resonance at 400 cycles per sec., we found that the difference between taking BC as a mean value and as the value at 400 cycles per sec. was so slight as not to justify the extra labour involved in computing this mean value; and, since it was desired to make the tests rapid, the 400 cycles per sec. value was adopted. I agree that a decibel scale would be preferable in this case. I cannot agree that all the receivers tested were bad—they were submitted for test as being distinctly better than most, and in fact were suggested as being patterns upon which a limiting specification could be drawn. With reference to Mr. Turner's remarks on Section (1) of the paper, I will give the following information regarding the valves used. The oscillator and modulator valves may be any small power valve such as the P240, and the output valve is a fairly large power valve such as the LS6A. The coupling condenser between the oscillator and the modulator, together with the bias of the modulator, are so adjusted as to give a linear relationship between the anode voltage of the modulator and the output current, and it is found that these values are not critical and, if once set, remain valid over a very large working frequency-range. The variation in frequency by means of a variometer inductance is a helpful suggestion, but is not quite so convenient in practice as a variable condenser. A linear modulation meter could be used, but is not suitable in the present case, since a much larger input would be required and a linear modulation meter can only be operated up to 70 or 80 per cent modulation. There is no difference in operating behaviour and accuracy, and, owing to the greater sensitivity of the square-law meter, it was adopted. There is nothing more to add to the information given in the paper regarding the details of the attenuator, which was fully described in my previous paper (*loc. cit.*). Capacitance effects to the screen were made small by careful calculation, and checks have been made to demonstrate that these potentiometers give reliable results at frequencies as high as 30 000 kilocycles per sec. The difficulty with a valve voltmeter as the final instrument upon which the accuracy of the measurement depends is, that for small currents the series resistance across which the voltage has to be measured must be large, which involves uncertainty at very high frequencies. It was more convenient to make this final measurement in terms of current, and, since such a current measurement is probably more accurate and requires no auxiliary apparatus, it is preferable to the valve-voltmeter method. The bridge described in the paper for measuring harmonics may be used to pick out individual frequencies if desired; but, if this were done, the measurement would become very involved and it is doubtful whether the information so obtained would be of much value in a specification. Grouping of the harmonics forms a simple means of assessing distortion, and is adopted in American specifications.

In reply to Mr. Barton, cabin A is 12 ft. × 8 ft. × 9 ft. high. For the testing of frame-aerial receivers, a small series resistance is inserted, the input e.m.f. being applied across this and a calculation made to allow for the slightly increased frame resistance. Similarly, if no earth connection is provided, a balanced condenser-system can be attached to give the required signal e.m.f. This has been done at 40 000 kilocycles per sec. when testing a short-wave frame direction-finder. Standard signal generators give a fair degree of accuracy, but the equipment described in the paper must obviously be superior in performance since it can be used to calibrate such signal generators. Although quite satisfactory for their purpose, much has to be sacrificed for portability and robustness. The apparatus described in the paper can readily be used for testing receivers having automatic volume controls. The reply to Mr. Barton's remaining remarks is contained in my reply to Mr. Cooper.

Mr. Scroggie raises the question of the value of the output load of 60 ohms. This value is determined by the attenuator network, and for the ratios used 60 ohms was a suitable load. One does not build an attenuator to match a valve, but to have as low a resistance as possible to give the required voltage range. High-resistance attenuators should be avoided at high radio frequencies owing to the difficulty of self-capacitance effects. The coils are chosen to have an inductance such that a maximum wavelength change can be covered on each range. Regarding the use of two cabins, the function of cabin A is to render outside field strengths negligible, and that of cabin B is to screen the oscillators effectively. Although, no doubt, some simplification of these screens could now be effected, it must be realized that the apparatus was built for many purposes other than testing broadcast receivers; unscreened apparatus may therefore have to be erected quickly in the large screened cabin B. This is quicker than building a complete screened set for every special measurement required. Mr. Scroggie's remarks on the harmonic bridge seem to be superfluous, as they describe what is done in the present bridge. His remarks on service areas show that he is thinking of a special type of sensitive receiver and not of a typical broadcast receiver designed for use within a service area. Regarding the testing of receivers without their associated loud-speaker, I have repeatedly urged that this type of comprehensive overall measurement is desirable, but a strong feeling has hitherto existed that a receiver should be designed to operate any loud-speaker and that compensation should not be resorted to. I prefer such an overall measurement and would welcome a request to test receivers in the manner suggested, by adding acoustic measuring apparatus to the existing electrical gear. I notice with pleasure that Mr. Scroggie agrees that at present a 5-kilocycle upper frequency limit is imposed by the present frequency allocations. The remaining points which he raises are dealt with elsewhere in this reply.

I am glad that Mr. Oliphant appreciates the object of the paper, namely, to stimulate interest in the standardization of test procedure. Regarding his remarks on the dynatron oscillator, I would point out that certain rough tests have been made on it and that this type of circuit appears to be quite satisfactory. The testing apparatus was constructed, however, before this work was completed and hence the dynatron oscillator has not been used. Doubtless, if more work were done, the dynatron oscillator might be shown to be preferable to the present triode generator. I am indebted for his correction of my figure of 40 per cent modulation.

I thank Dr. Smith-Rose for his remarks, which do not call for any reply but add useful information to the paper and in themselves answer several points raised by speakers.

I have already partially answered the point raised by Mr. Brown in regard to checking the attenuator at high radio frequencies I would add, however, that it is possible to establish the accuracy of the system by cross-checking on various attenuator settings with different current values and at different frequencies. The results of numerous such tests establish the fact that the order of accuracy of the present design is well within 1 per cent. I have already replied to the point as to testing the receiver with the loud-speaker. The next point raised by Mr. Brown is covered by the sensitivity classification. If the ganging were bad, the receiver would not give satisfactory results in the particular class in which it gave optimum results, and it would have to be classified separately for different wavelengths. This would immediately demonstrate its failings as regards sensitivity. The necessity for testing at the maximum output is due to the fact that all tests must be made with some definite signal; and since one of the tests, viz. purity factor, demands maximum output, it is preferable to maintain this output for all the other tests. I disagree with Mr. Brown as to the best method of testing selectivity. The case cited is not normal but is obtained when a receiver is used to receive a weak signal in the presence of an interfering strong signal. This condition is important in assessing the properties of a powerful and selective receiver, but for the vast majority of receivers the interference is not greater than the desired signal. Hence, if the output obtained at the desired frequency is known, and the output at some frequency removed slightly from the desired frequency is also known for the same setting of the receiver, this information enables a precise picture of the operating conditions to be made. The introduction of a suitable numerical coefficient in the definition of selectivity meets Mr. Brown's final criticism; such a coefficient would be determined by a Standards Committee.

I am inclined to agree with Mr. L. B. Turner that the method which I have adopted of defining selectivity does not sufficiently grade performance. A suitable numerical coefficient would, however, be preferable to expressing a poorly selective receiver by a higher figure than a good receiver. I am afraid the latter method would lead to confusing results, particularly from the advertisement point of view.

In reply to Mr. Cooper, the harmonics which are always present are almost certainly due to the detector. It would be nearly impossible to test selectivity in the manner suggested. If two signals were imparted to the aerial system and the wanted and unwanted signal were varied in value and frequency over a range commensurate

with actual working conditions, the mass of results would be prohibitive. Admittedly, selectivity does in practice depend upon the precise method of excitation, but any specific conditions can be obtained by calculation from the simple selectivity characteristic suggested. I cannot see how a poorly designed portable generator can give results superior to those with a generator in which every point has been most carefully examined and where efficiency and precision have not been sacrificed for portability.

In reply to Mr. Nisbet, I prefer to take the line BC at the mean value or at some mean frequency such as 400 rather than at the peak value, as the latter method would not differentiate between a receiver with a sharp resonance at some frequency and one in which the resonance rose to a high value over a large band width. The point in regard to selectivity has already been answered in my reply to Mr. Cooper. The manufacturer would classify the receiver, and the testing laboratory would give figures for that class, or, alternatively, the laboratory could give figures for more than one sensitivity class. I am inclined to agree that the numbering and lettering could be improved. Naturally, these points are all open to discussion.

The encouraging and helpful remarks of Dr. Rayner are welcome. It must be realized that many of the suggestions put forward by several speakers would involve a prohibitive amount of testing, and the results would probably be of little practical value. The problem which I have attempted to solve is that of providing a simple test schedule to act as a basis for discussion, and although in detail it may be subject to many academic criticisms, I maintain that the essential points do form a definite criterion of receiver performance upon which present-day receivers could be approximately graded according to their relative effectiveness.

Most of the points raised by Mr. Voigt have already been answered. I am not very favourably disposed towards a system of merit figures which inverts the performance. A more steeply graded system is possibly desirable, but I feel that the perfect receiver should be assessed as 100 per cent and not zero.